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ABSTRACT

The nature of linguistic variation is examined, particularly the ways in which phonology, morphology, syntax, and other aspects of language vary according to social and situational contexts. A distinction must be made between a difference in frequency of a linguistic variable that carries meaning, and a difference in frequency which carries no meaning but is the manifestation of the more or less frequent usage of a form in a situational context. Although the analysis of variation in phonology by defining phonological variables can be accepted as contributing to a better understanding of the kinds of information that differences in form may be conveying, the parallel extension of the notion of variable to non-phonological variation may in many cases be unrevealing. One difference between phonological and non-phonological variables is that phonological variables with social and stylistic meaning need not have referential meaning, while non-phonological variables are defined so that all variants of this variable must have the same referential meaning. It is proposed that the notions of sociolinguistic variables and variable rules be restricted to the analysis of forms that communicate social and stylistic significance through their variation. (AM)

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Where Does the Sociolinguistic Variable Stop?

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RELEVANCE TO EDUCATIONAL PRACTICE

There are different theories of the causes of and the nature of linguistic variation. Lavandera's paper deals with the nature of linguistic variation, especially the ways in which the various aspects of language--phonology, morphology, syntax, etc.--vary according to social and situational contexts. Educators need to realize that linguistic variation is universal and should be expected in the speech of students. An awareness of the social and ethnic backgrounds of students contributes to a better understanding of the nature of linguistic variation in their speech.

WHERE DOES THE SOCIOLINGUISTIC VARIABLE STOP?

Beatriz R. Lavandera

In 1972, Labov described what he called the fundamental sociolinguistic question as the one "posed by the need to understand why anyone says anything" (1972: 207). Clearly, the aim he is establishing for a sociolinguistic theory of utterances is very different from that of specifying the form of a grammar that generates all and only the well formed abstract sentences of a language. For the sociolinguistic question thus characterized, descriptive adequacy, although necessary, is not sufficient. The "why" question is not only a requirement of explanation, but it can also be read as "what for." What does anyone say anything for? I think we can safely say that this question places sociolinguistic analysis in a functional framework. If sociolinguistics looks for answers to the "why" of saying something, it is seeking functional explanations. The discussion that follows can be meaningful only to those who recognize this need.

It is from this perspective that I will evaluate the study of variation conducted thus far. I hope to show that while the analysis of variation in phonology by defining phonological variables can be accepted as contributing to a better understanding of the kinds of information that differences in form may be conveying, the parallel extension of the notion of variable to non-phonological variation may in many cases be unrevealing. (1)

I want to stress from the outset that I am not suggesting quantitive data should not be handled beyond the level of phonology; rather, I will be assigning a different status to such data because they in turn need further interpretation: they do not in themselves constitute a definitive analysis. Equally important, I consider that the research carried out so far on syntactic variation has been extremely valuable, among other reasons because it makes possible

the kind of study I am here carrying out, the examination of the different nature of phonological versus non-phonological variation.

In 1972 Gillian Sankoff presented a paper called "above and Beyond Phonology in Variable Rules" in which she posited that "the extension of probabilistic considerations from phonology to syntax is not a conceptually difficult jump. Whenever there are options open to a speaker, we can infer from his or her behavior an underlying set of probabilities" (58). She accompanied her suggestion with three examples of non-phonological variation. One is the placement of the future market in New Guinea Tok Pisin, a study which she carried out with Laberge. They found that the future marker bai is variably placed before or after the subject NP, but failed to find either generational differences or differences among individual speakers. They reported instead that there are a series of syntactic constraints, some of them categorical and others variable. They make a clear distinction between the effect of all pronouns that follow bai as opposed to that of the third person pronoun and nonpronominal NPs that generally precede it. The dividing line suggests a semantic difference, but the authors do not attempt to interpret the syntactic constraints they were able to uncover in semantic terms. However, they acknowledge a stylistic factor: some applications of the bai-movement rule take place to "indicate particular emphasis on the pronoun subject to the exclusion of other people" (48).

The second example Gillian Sankoff provided in her paper has been studied repeatedly by members of the Montreal group of sociolinguists: the deletion of the complementizer que in Montreal French. Again in this case there are no social or stylistic constraints reported, but the author provisionally concludes that "the presence or absence of que is differently allowable for different grammatical constructions" (54) (by which she means comme que versus quand que, pourquoi que, ce que, etc.). She adds that this difference in allowability

"takes place in a way that is clearly important to an understanding of the grammar of French" (54). Another difference she reports has to do with the allowability of est-ce with quand and comme, she suggests that it "might stem from the fact that comme can never be used in a direct question; whereas, quand can" (54). At this stage, all that matters is to call attention to the fact that the first two examples of Gillian Sankoff's 1972 paper involve syntactic constraints; no social or stylistic significance is reported.

Let me point out that when higher and lower scores of a variable are directly correlated with higher and lower positions on a socioeconomic scale, those scores are interpreted as carriers of social significance. In the same way, when higher and lower scores of a variable are directly correlated with higher and lower positions on a scale of formality of the context, those higher and lower scores are the carriers of stylistic significance. On the other hand, no study which reports different scores for different age groups has interpreted these scores as carrying any kind of "generational significance" or "generational meaning." Instead, generational differences are interpreted, correctly as far as I am concerned, as indicators of usage change. Sankoff's third example is of this last type. It is taken from a longer and very rich study by Laberge (1977). Gillian Sankoff singles out the variation found in the use of Montreal French indefinite on. "Contrary to the que example, this example shows a dramatic and rapid usage change" (58) and a variable rule can be written with increasing input probabilities for younger speakers.

But if this is the case, I want to establish a distinction between a difference in frequencies which in itself is the carrier of meaning, be it social or stylistic, and a difference in frequency which is not a device that communicates some information, but simply the manifestation of more or less frequent usage of a form in a situational context or in a social group. Let me insist on this difference now. If we take a stereotype instead of a sociolinguistic marker,

let us say the form "wiped out" as opposed to the standard form "exhausted," we have in each of its occurrences that the form "wiped out" has the stylistic significance of "informal speech." At the same time, we have a correlation of higher scores of the form "wiped out" in informal contexts and lower scores of the same form in formal contexts. It is not the case, however, that differences in scores alone are significant, rather, the forms themselves contain features which carry differences in meaning. The differences in frequencies between the different contexts are mainly derived from the fact that an informal variant is more appropriate for informal contexts than for formal contexts. I will want to distinguish, however, between frequency relationships which are devices of the language to convey non-referential information, and frequency relationships which are the consequence of the compatibility between the referential, social, or stylistic meanings of some forms and the different contexts in which they may occur.

To sum up, the three examples presented by Gillian Sankoff to support her call for the study of syntactic variation were not cases in which the variation seemed to be the carrier of social and stylistic meanings. The constraints reported were either syntactic or, as in the third example, represented evidence for an ongoing change in usage within the community.

In order to state what I think we are losing in extending the concept of variable to "whenever the speaker has an option," let me first outline what I think has been achieved by introducing the notion of "linguistic variable" to the analysis of phonological variation. I think the gains in terms of the understanding of language, in terms of a criterion of explanatory adequacy were at least twofold. In the search for an answer to the question "why anybody says anything" Labov presented evidence in 1966 for two important facts of language form and language function. In showing that differences in form which had so far been analyzed as unmotivated or free, that is, referentially

meaningless, were in fact carriers of some significance, social and stylistic, he provided specific evidence for the hypothesis that most if not all differences in form convey some information. He was able to deal with what was thought to be clear evidence that some differences in New York pronunciation had no distinctiveness. Furthermore, he provided the means to continue reexamining more of the so-called evidence of free variation. It could now be shown for more cases of differences in form that they correlated with differences in meaning, once the notion of meaning was extended to include social and stylistic significance. Although social and stylistic meanings had of course been recorded throughout the history of linguistics, that recognition had not principled linguistic analyses which were always done on the basis of referential meaning, leaving all other meanings in a subsidiary, derived and fluctuating status. This promotion of stylistic and social information to an equal level with referential information was what Hymes recognized in Labov's work and what he developed in his theory of a grammatical and stylistic component (Hymes 1974: 149).

Another significant revision of the accepted theory of language was the recognition of the existence of another kind of formal carrier of significance, that is, the frequency relationship. As I said above, it is higher or lower scores of a variable which are correlated with higher or lower values of a socioeconomic index and/or higher or lower positons along a scale of formality in the context, not the presence or absence of a variable. As a matter of fact, for cases of inherent variation it is reported that there are no speakers who never use a variant nor are there any who always use it. Also, a strict co-occurrence has to be distinguished from the defining property of a variable, which is <u>covariation</u>. In Weinreich, Labov, and Herzog (1968) we read: "Quantitative evidence for <u>covariation</u> between the variable in question and

some other linguistic or extralinguistic element provides a necessary condition for admitting such a structural unit. Covariation may be opposed to strict co-occurrence, or co-occurrence may be conceived as the limiting case of variation" (169). It is not therefore which form is chosen in any particular occurrence but the frequency with which one form is chosen over another alternative form which, when correlated with some other linguistic or extralinguistic element, takes on significance.

The study of variation thus began mainly as the study of social and stylistic variation. According to Labov, "social and stylistic variation presuppose the option of saying 'the same thing' in different ways: that is, the variants are identical in reference or truth value, but opposed in their social and/or stylistic significance" (1972: 271). It is already clear why phonological variables were better candidates for the first studies of linguistic variation than other kinds of options in the language. Laughing and laughin', or gɑɹd and gɒ:d can more convincingly be shown to be used to say referentially the same thing than any pair of postulated synonymous syntactic constructions such as The liquor store was broken into vs. They broke into the liquor store. Such a syntactic difference, as we can see in Labov and Weiner's (1977) study of this as a variable requires quite an ingenious dismissal of possible differences in meaning. Also, as I will try to show later, it forces a fragmentation of grammatical facts which strikes me as counterintuitive. In any case, since social and stylistic variation is to be sought for variants which are identical in referential value, phonological variables seemed the safest ground to start from. What I will be questioning is whether that ground of clear semantic equivalence can be abandoned to carry out the same kind of study of variation for syntactic or morphological units which have to be proven to mean "the same," to be treated as evidence of variability and furthermore, whether semantic

equivalence must in fact be a requirement at all. Notice where the source of the difficulty lies: units beyond phonology, let us say a morpheme, or a lexical item, or a syntactic construction each have by definition a meaning. They are not, like phonemes, empty of referential information: on in French has a referential meaning in each of its occurrences, which we can describe or label differently according to our analysis, for instance, "exclusive indeterminate referent" but since it is recognized as a grammatical form of the language, it has one (or more) meaning(s). In the same way, when Sankoff and Thibault (1977) deny any referential distinction in Montreal French between être and avoir for some of the contexts in which both forms occur as auxiliaries, they are not saying that either être or avoir fail to carry referential meaning, they attempt only to show that the referential meaning of both forms in those context is the same. To put it differently, they show that in some contexts the choice of avoir instead of être is not promoted by the need to mean one thing instead of another, rather it is affected by the speaker's place in the linguistic market and by the different probabilities introduced by the lexical item of the main verb. So for the analysis of variables above the level of phonology, there is no question of getting rid of referential meaning. It cannot be said that such variables do not have referential meaning. However, it has been agreed that the referential meaning of all the variants of a non-phonological linguistic variable must necessarily be the same (Labov and Weiner 1977, Sankoff and Thibault 1977, Laberge 1977, etc.).

Thus we see that the first difference which can be pointed out between phonological and non-phonological variables is that phonological variables which can be shown to have social and stylistic meaning need not have referential meaning, while non-phonological variables are defined so that

even when they do carry social or stylistic significance, although they have referential meaning, this referential meaning is the same for all its variants treated as evidence for a single variable.

I will read a paragraph from Labov's 1966 study of the English of the New York City Lower East Side in which he justifies his selection of variables for study: "The most useful items are those which are high in frequency, have a certain immunity from conscious suppression, are integral units of larger structures, and may be easily quantified on a linear scale"(49). Labov concludes: "By all these criteria, phonological variables appear to be the most useful"(49). I agree, but I would add that they also appear to be the most useful because the definition of phonological variable does not require the extremely difficult and often not totally convincing task of showing that all variants of the variable have the same referential meaning. I will discuss later the possibility (considered and discarded by Labov in his study of get/be in the passive) that the variable may be defined, even if its variants do not say the same thing.

For those of us who, with a great deal of enthusiasm, undertook the task of extending the study of variation beyond phonology, the main difficulty seemed to be the elicitation of a sufficiently high number of forms in cases where the variables under examination were relatively rare forms of the language. It really seemed as though the challenge lay in the elicitation process. However, it turned out that that was not the major difficulty. What is much more difficult is to define the relevant environments for the variable. By way of illustration, I will discuss in some detail Sankoff and Thibault's analysis of the être-avoir variable (1977), and Labov and Weiner's analysis of the passive variable (1977).

While Labov had previously directed his main effort in his studies of

phonological variables to demonstrating that differences in form so far regarded as meaningless were in fact the carriers of social and stylistic meaning, one of his latest papers on variation, co-authored with Judy Weiner, seems to be aimed at proving the opposite, that some different forms are used to say the same thing, that is, that forms referentially identical carry neither stylistic nor social meaning, nor are they semantically motivated, the choice being constrained almost entirely by syntactic factors.

First of all, it must be made clear that the interest of this kind of study, which I would never deny for the general theory of language, is very different from the interest the earlier studies of social and stylistic variation had for a "realistically social linguistics" (Hymes 1974: 193-209). In his early study of Martha's Vineyard when listing what he calls "the most useful properties of a linguistic variable to serve as the focus for the study of the speech community, "Labov gives as the third property that "the distribution of the feature should be highly stratified: that is, our preliminary explorations should suggest an asymmetric distribution over a wide range of age levels or other ordered strata of the society" (1972: 8).

In the study of the passive variable, after the first steps of the analysis, it appears that for these effects "none indicate that external factors have any sizeable influence on the choice of active vs. passive in agentless sentences. Whatever the passive is, it does not appear to be a prominent sociolinguistic variable" (1977: 12). The purpose of studying this linguistic alternation is therefore not that of studying a speech community.

I find it necessary and illuminating to carry out these kinds of studies where a linguistic option appears to be socially unconditioned and semantically unmotivated, but I also think that they show the limit of the applicability of the notion of linguistic variable. I realize that Labov and Weiner admit that

it is not a sociolinguistic variable since they could not establish a clear social stratification of the option, but they insist on calling it a variable, and they qualify it as "a well-established variable in English" (1977: 11).

I just want to point out at this moment that the terms "sociolinguistic variable" and "linguistic variable" are used interchangeably in studies of variation, the third useful property I have cited from Labov's Martha's Vineyard study was assigned to what he there called "the linguistic variable." It is not then that we are in the presence of a new concept, the linguistic variable which carries no meaning, which therefore does not signify by means of relative frequencies, and which is different from the sociolinguistic variable. The concept of "linguistic" or "sociolinguistic" variable is the same one.

Now, the notion of the variable was originally introduced to account for those cases of variation which could be shown to carry social and stylistic significance, and furthermore, where social and stylistic significance was manifested by consistent differences in frequencies which covaried with other linguistic and extralinguistic factors. The single characteristic which is preserved in this non-sociolinguistic variable is that the frequencies covary with other elements, in this case with forms of the surface structure. The variation is said to be meaningless, in all three dimensions of meaning, and consequently, the frequency with which one form occurs as opposed to another does not convey any information. If the problem is interesting, and I think it is for the general theory of language and for the description of English, it responds to concerns very different from those of the phonological studies of variation. Furthermore, although the term "variable" is now being used to refer to any form of option, it was originally proposed for a certain definable kind of linguistic element.

Labov and Weiner's study of the passive variable is also a very useful example to discuss what is entailed methodologically in the isolation of a non-phonological variable. First of all, the authors explicitly state the criterion of "saying the same thing" or of having the same truth value. They also make it explicit that if this were not the case, this would be "an unlikely site to apply methods to analyze the constraints on optional (or variable) rules, which are basically used for analyzing ways of saying the same thing" (2). Therefore they proceed "upon the assumption that active and passive have the same meaning in a truth-definitional sense" (2). And they add: "We believe that the results of our analysis do much to justify that initial premise" (2).

A characteristic of this kind of study of syntactic variation is that the definition of the variable requires a series of preliminary steps directed at eliminating all the cases in which the two alternant forms contrast, i. e. do not say the same thing.

This strategy of setting aside more and more contexts where both alternants occur but do not say exactly the same thing is for example reported in Sankoff and Thibault's analysis of the French auxiliaries être and avoir (1977). First of all, they have to set aside the cases where an adjective can be substituted for the participle, être is in that case not an auxiliary but a copula, and avoir is unacceptable. Within the auxiliary set itself, avoir and être do introduce an aspectual distinction, être occurring in [-completed] actions to the exclusion of avoir, which would change the meaning to [+completed]. Finally, être and avoir vary only in the context of aux + pple for [+completed] actions. Within this laboriously defined context, they attempt to show that être and avoir have the same meaning. Notice that the context thus isolated in terms of this variation has no independent motivation for separate consideration. It becomes evident that for être and avoir, which are clearly distinct

forms with different meanings in the grammar of French, there is one context in which they seem not to introduce any difference in meaning and where they vary according to social and lexical constraints. That every language has neutralized constructions [in a referential sense] is, of course, a very old observation. I hesitate, however, to analyze these cases as "variables" unless two conditions hold: (1) they can be proven to be the carriers of some nonreferential information, to have social and stylistic or other significance, as is the case for être and avoir but not for the passive variable; and (2) they prove to be a kind of device of the language similar to the phonological variables, that is, elements whose defining property is a quantifiable variation and for which the frequency relationships are the very signals of those significances.

There is another kind of fragmentation which is performed in this kind of study. Although the variants are defined as "alternative ways of saying the same thing from a truth-definitional point of view" (Labov and Weiner 1977: 6), not all the alternative ways of saying the same thing are grouped within the same variable. Labov and Weiner offer a justification for one of these cases of exclusion of an alternant in the discussion of the passive variable. Clauses containing verbs with sentential objects are excluded because the alternant of, for instance, They say that times are hard with generalized they would be That times are hard is said, which is obligatorily transformed into It is said that times are hard. Labov and Weiner say: "We found that in accordance with our intuitions ... extraposition was categorical, and extraposed sentences like It is said that times are hard involve changes of surface structure that are incompatible with the constraints to be considered" (7). Although this may be a valid justification from the point of view of the method of analysis employed, it is also true that They say that times are hard

and It is said that times are hard are not set aside by the authors because they say different things, but rather the second alternant is excluded from the analysis on grounds of methodological convenience.

Can a variable be defined for less than its complete set of alternants? I find that for syntactic variables this would have to be the case since it will often turn out to be impossible to consider all the ways available of saying the same thing if all we are applying is the truth-value criterion. Very often, as in the case I just quoted from Labov and Weiner, some of the alternants will be the outcome of transformations "that are incompatible with the constraints to be considered" (7).

I would now like to consider the possibility of relaxing the requirement that "alternating forms say the same thing" and look at the social or stylistic conditioning of forms which do differ in meaning. One of the reasons for restricting the study of variables to surface variants is the fear of providing arguments which can be used irresponsibly to support ethnic, racial and class-based prejudices. (2)

For the problems I am examining here, the "dangerous" hypothesis would be that forms which clearly differ in referential meaning are nevertheless at the same time socially and stylistically stratified. This kind of evidence would show that different social groups exchange different types of messages for which they make use of forms with different meaningful structures. Instead of leading to the conclusion that there are different conventional ways of communicating "the same" referential effect in the different sectors of the speech community, this evidence would be used incorrectly to attribute to some groups the inability of thinking certain meanings.

That this consequence is feared is stated explicitly by Laberge and implied by Labov. However, I will argue that the first hypothesis is perfectly

reasonable and that the misinterpretation of the evidence will have to be prevented by further evidence and argumentation against these kinds of prejudices. In a January 1977 paper entitled "The changing distributionof indeterminate pronouns in discourse" Laberge says in her conclusion that the variants on and tu/vous, in the contexts in which she has studied them, are "fulfilling identical semantic functions" (she means referential functions in this case). She explains why this has to be the case: "Any analysis claiming that the use of tu and vous is tinged by the determinate second person origins of these clitics ... falls necessarily into the trap of espousing such discredited notions as that working class speackers are less capable of abstract discourse, not having access to genuinely indeterminate forms" (16-17).

First of all, since working class speakers make some use of the form on even if they prefer the form tu/vous, the evidence would not show an impossibility of using it, but a preference for the other forms. But more crucially, nobody has as yet proven that the kind of more general, or as Laberge calls it, more abstract meanings, reflect a cognitive or a communicative superiority. The prejudice consists in believing that since upper class speakers apparently make more use of these more referentially general (mislabeled "abstract") linguistic categories, these meanings can be taken as signs of a greater intelligence or of a more effective communication. It has neither been proven that upper class speakers really make more use of this kind of meanings nor that this linguistic behavior would necessarily be "better." Moreover, as we all know, the appropriateness of one form of expression in place of another, let us say more depersonalized vs. more personalized discourse, depends on the speaker's aim. A good particularized example of a concrete situation can have a better chance of winning an argument than a very abstract enumeration of general factors.

Labov provides another intersting example of validating a claim of referential equivalence precisely by showing that the variable is socially or stylistically stratified. This form of argument is just the reverse of what we have so far considered. He is discussing the synonymity of the constructions I got arrested/I was arrested. As a final argument to decide against the claim that the causative meaning of get is present in the get-passive, he reasons: "On the other hand, if we argue that [I got arrested] and [I was arrested] mean something different, there is an even stranger consequence. It is well known that the 'get-passive' is more colloquial than the be-passive; in fact, many people deny using it altogether. Though no systematic study has been made, there is reason to believe that its use is stylistically and socially stratified. Is it the case that people think more causally when they are talking more informally?" (1974: 62). This whole argument comes from the behavorist doctrine that "thought" is just "internalized speech."

I would say that we do not as yet know what talking more informally implies. Aside from the fact that talking informally is defined by less attention being paid to speech, we do not know whether the opposite choice, that of talking "formally" may not involve some requirement of reifying or concretizing all of the information by establishing "distance" from speaker and hearer. Thus, not only will third persons be preferred, but perhaps also general subjects which do not specifically imply the participation of the speaker or hearer will also be favored, and so forth. This will have to be established separately for different cultures, although there may turn out to be some universals. Talking more causally can be more appropriate when talking informally and furthermore, "talking" does not necessarily imply "thinking" more causally. This has to be investigated openly.

There is no reason for continuing to consider the idea of a different

distribution of structural meanings in the different social groups to be a "discredited notion" so long as these differences in distribution are not evaluated in terms of more or less intelligence, more or less expressive power, more or less verbal ability.

I need to make a very strict distinction at this point among the following: carrying out quantitative studies of variation, writing variable rules, and defining linguistic variables. I have pointed out the difficulties I perceive in defining non-phonological variables, since as we have already discussed, all the cases studied involve referential meaning, regardless of whether or not we accept the condition that the referential meaning be the same for all the alternants. I realize that similar difficulties might be raised for the description of phonological variables and the writing of phonological variable rules. It is not my intention to discuss these now. However, I do think that the absence of referential meaning makes an important difference and that this difference is not only methodological but that it has shown up in the fact that phonological variables have proven to be far better candidates for conveying the restricted kind of sociolinguistic information that has been analyzed than non-phonological variables.

I want to introduce a distinction between "variables" which are elements of the language and the carriers of social and stylistic significance, and "variables" which are simply heuristic devices to group alternants and subject them to quantitative analysis. But it should remain clear that this distinction does not imply abandoning the attempt at carrying out quantitative studies of linguistic behavior "whenever the speaker has a choice."

I propose to restrict the notions of sociolinguistic variables and variable rules to the analysis of forms which communicate social and stylistic significance through their variation. Elsewhere quantitative statements can

still certainly be treated as data which call for interpretation and probabilistic rules can still serve as heuristic devices.

My view is that we should continue the development of probabilistic models for all levels of linguistic analysis while regarding the regularities and tendencies illustrated by these probabilities as a richer kind of data subject to formal and substantive explanation.

FOOTNOTES

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(2) Some of the motivations for restricting the study of variables to surface variants which fulfill identical referential functions are the same kind of well-intentioned reasons, pointed out by Hymes in his Foreword to Swadesh's last book (1972: vii), that led to the adoption by American linguists and anthropologists since the First World War of an egalitarian and relativistic point of view which excluded the connection of diversity intrinsic to language with sociocultural diversity. In both cases, the fear of providing arguments which can be used irresponsibly to support ethnic, racial and class-based prejudices inhibits the exploration of hypotheses which in other senses are plausible.

In the older tradition, linguists pointed out the ability of every language to code every referential distinction in some way, all languages being ultimately equal in referential power, though differing in structure, in the way they make structural connection with the universe of reference.

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